

September 18, 1956

Dear Gaylen:

I had hoped to answer your letter more promptly, but we've had an unprecedented spate of visitors-- not excluding the Sussman's, whom we found ensconced in our parlor when we came home last night, after having taken another couple (from Australia) to dinner. We haven't had a free evening in I don't know how long.

Your current findings are too perplexing to think about from this distance. I don't recall-- have you studied the effect of UV on the dissociation of (anomalous) heterokaryons. How far have you been able to confirm Seronti's findings (with his strains)?

Your report was excellently phrased-- in fact, I thought at first you had written an ms. for publication. On balance, though, I would not strongly recommend that you do publish this material until you have cleared up the possible discrepancies among various strains.

I don't understand your greatest perplexity, that you had gotten "H-H" out of "U-U". Do you mean in backcrosses, or as "new mutants"?

A few comments in the report, as if it were an ms:

I question whether this is the appropriate form; customarily, the place where the work was done and sponsored should appear as the address, and a footnote for the subsequent migrations of the author.

B&L 1956a is a trivial reference: why bother with it. I would say almost the same for Sz&B, except to lean over backwards; hopefully, they will have something more respectable in print in due course.

NRRL-B1257 was furnished by the courtesy of Dr. K. Raper (then at Peoria): that citation means ~~something~~ in identifying the culture; getting it from me doesn't. You could rely more fully on (I) for the methods, e.g., ~~the~~ deFonbrune reference is superfluous. The point of that ~~inference~~ in I wasn't the manipulator but the oil chamber technique.

Do you need the data of table 3? The experiment is worth mentioning in passing but hardly needs emphasis, since the spore isolations are direct and decisive. If you have the likes of a table 3, its "statistics" should be explicit. (e.g., what was the variance in replicate plates).

If you should feel strongly about disseminating your incomplete inferences, you might want to consider condensing this into the compass of a Note for J Bact. If I had to pass on this for editorial review, I'd be unhappy about taking too much space for a tentative report.

The new girls (Ann Cook and Jacq. St Clair) have just gotten under way; Bob returned from Spain today, and Mari promises to be back in the lab the end of the month. The lab. still seems half-empty on your side. But the Orskovs will be here in just a month, and Heumann (Rhizobium) end of Nov.

The most interesting things the last couple of weeks have turned out to be a retake of Dienes, i.e., the proliferation of protoplasts to form L-colonies. We've gradually been learning how to do this with E. coli, and finally with K-12 which proves to be somewhat trickier than others. Now the question is what to do with it, but we have some notions about that.

Maury told how well you've been assimilating the department-- keep it up.

Yrs. sincerely,

Joshua Lederberg